DEVELOPMENT OF AN INTEGRATED FARMING SYSTEMS RESEARCH NETWORK IN INDONESIA*

by

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INTRODUCTION

Farming systems research is neither a new term or a new methodology. Many of the techniques used in farming systems research follow from the farm management research developed and carried out by agricultural economists in the early 1900's. As brought out by Simmonds (1983), the international agricultural research centers (IARC's) turned to various forms of farming systems research soon after the realization that the sucesses from the Green Revolution seemed to be restricted to two commodities (rice and wheat) in farming areas with adequate water and the necessary infrastructure for delivery of the appropriate set of inputs and markets for the resulting increase in production. The focus of analysis has evolved rather rapidly from multiple cropping research programs to cropping systems and, ultimately, to farming systems.

As in many other developing tropical countries, the application of farming systems research related methodology is not entirely new to Indonesia. Farming systems research has been carried out through the many farm management case studies and area surveys. The most recent major example of farming systems oriented research has been the various studies done by the Agro Economic Survey/Rural Dynamics Study in which all household enterprises, including non-agricultural income sources are analyzed. Although it has not been identified as such, the present Patanas or National Panel of Farmers Research Program of the Center for Agro Economic Research (CAER) can be considered an adaptation of the farming systems research approach. In a more direct way, farming systems related research had its origins in Indonesia with the IRRI-sponsored rice-based cropping systems program which evolved into an important component of the research program for the food crops research institutes of the Agency for Agricultural Research and Development (AARD). (Syarifuddin *et. al.*, 1984).

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METHODOLOGY FOR FARMING SYSTEMS RESEARCH IN INDONESIA

Farming systems research is a useful approach that can contribute greatly to the effectiveness of agricultural research in developing countries such as Indonesia.

The application of farming systems may vary between countries. However, the basic definition of farming systems research is fairly constant. A sample definition is given by Gilbert *et al*, (1980):

"In FSR the farm is viewed in a comprehensive manner, and constraints in the farming systems, research priorities, and strategies for improvement are evaluated in terms of the whole farming system."

A similar definition is provided by the Technical Advisory Committee of the CGIAR (1978):

"In its broadest sense, FSR is any research that views the farm in a holistic manner and considers interactions in the system."

The recognition of the interactions in the farming system is defined as the farming systems perspecrtive (FSP) by Byerlee *et al*, (1982).

Farming systems research can be further differentiated between "upstream" FSR and "downstream" FSR. Gilbert (1980) said upstream FSR develops prototype solutions through experiment station research to overcome general constraints in the zone where upstream research is being conducted. Downstream research is described as involving the farmer in the research process by way of his contribution to present knowledge and by "on farm testing" of newly developed technologies.

Over the years a fairly standard methodology has also been developed for FSR. The general framework indicated by Gilbert (1980) and Zandstra (1981) includes four stages or sets of activities: (1) description or diagnosis of the present farming system, (2) design of improved systems, (3) testing of improved systems, and (4) extension of improved farming systems.

One of the consistent features of farming systems research is the multi-disciplinary research approach. The advantages of a multi-disciplinary approach are many. However, the implementation of a good multi-disciplinary research program is often difficult. As brought out by Chong and Swenson (1983), the application of multi-disciplinary research should be more easily accomplished in Indonesia. For socio-economic reasons, the research programs for most of the institutes and centers of the AARD are usually carried out by a set of researchers representing different levels of capabilities and rank in the institutional structure. The result is a "group research" approach. This experience implies the ability of researchers to work together on a given problem area. This experience should make it easier to combine necessary disciplines for farming systems research.

The need to work together in a multi-disciplinary fashion among farmers, researchers and extension workers to improve the welfare of individual farm families is recognized. However, the state of development of farming systems

research in Indonesia is constrained by factors such as the state of development of methodology and availability of resources, particularly manpower.

The determination of desirable and achieveable farming systems required to support the national development goals consistent with the farmers set of goals is the main objective of farming systems research in Indonesia. Until now, the focus of cropping systems/farming systems research has been on the farm or micro level. In order to better allocate the limited government resources and natural endowments, more attention needs to be focused on regional and national policy variables. In this context, determination of the appropriate combination of farm enterprises must take greater account of regional environmental characteristics such as presence of river basins, irrigation systems, dry-land farming conditions and socio-economic factors which affect their comparative advantage.

One of the implications of this reorientation is greater cognizance of the differences in the economic, social, climatic and other parameters between regions of the country. As can be seen in Figure 1, there are differences in the relative levels of resource endowments between Java and the outer islands.

	LAND	LABOR	CAPITAL
JAVA	—	+	+?
OUTER ISLANDS	+	—	—?

Figure 1. Existing relative resource endowments between Java and the outer islands.

Another factor which must be taken into consideration is the stage of economic development of the agricultural sector. As agriculture moves from a subsistence-oriented production system to a more commercial-oriented system, the level of integration with the other sectors of the economy increases as well as the ability of government policy actions to direct the production and overall development of particular enterprises or regions of a country. The need for policy-oriented research is becoming more appreciated. The research includes the consequences of farming system technology on agricultural production and marketing and the need to minimize the evolutionary changes in farming systems development to achieve the expected scenarios.

ORGANIZATION OF FARMING SYSTEMS RESEARCH IN INDONESIA

It is essential to begin from the underlying institutional base when analyzing an organization. The organization is a projection of the subjective institutional

base into the objective and rational dimension. Many times we fail to build an organization simply because we do not take the underlying institutional base into account. An organization without the necessary institutional base is like a body without life or a computer without a program. This is one of the biggest reasons for the failure to build a new and viable organization. This is true for modern organizations introduced in the traditional village society and for modern organizations within most modern technocratic societies.

The same situation arises when we consider an attempt to organize farming systems research in Indonesia, or more particularly, within the AARD. The general bias is usually attached to our over-reliance on the existing bureaucratic system, believing that the bureaucracy can make the accomodation. The normal reaction is to form a "steering committee" consisting of top level bureaucrats, and directly below, "organizing committees" which are given the responsibility of operation.

There is nothing evidently wrong with this type of organization. Yet, it does not serve all circumstances. This kind of organization is useful when a short run activity is the main objective, such as a seminar. However, a different kind of organization is required to support long term activities such as farming systems research.

The only institutional base on which one can depend is the "farming systems institution" living within the research community. Yet the "scientific institution" is relatively weak. In the research tradition "departmental scientific institutions" are much stronger, i.e. scientific institutions based on a crop specialization or a disciplinary specialization.

It is very difficult to build a strong organization where the underlying institutional base is weak. Special effort is needed to strengthen both the organization and the institutional base. This is analogous with an attempt to build cooperatives in rural areas. The same problem is also faced in our attempt to set up career programming for researchers, as the underlying "researchers institution" is at most, weak, or does not exist.

Given this background, the following issues are suggested as being important in the development of a farming systems research organization in Indonesia:

1. The strengthening of the institutional base requires the inculcation of the idea that farming systems are not the sum of technology components. They are a synthesis which needs to be handled by a group of professionals on the subject.
2. The emergence of farming systems professionals must be encouraged within each research institution. The lack of farming systems professionals is one of the primary reasons for much of the irrelevant research carried out in agricultural development. It is suggested that only farming systems professionals can effectively interact with economists and resources specialists to generate appropriate agricultural technology related to agricultural

development. It is essential that the farming systems professionals be acquainted with regional and agricultural planning.

3. It is particularly important for the farming systems professionals within the AARD to be united into one professional organization rather than a bureaucratic organization.
4. The core members of the farming systems research organization must be professionals in farming systems research. The other members can consist of subject matter specialists and staff from the offices of the Directorates General and universities.

A prefunctory review of the research programs of the various research centers and institutes would indicate that most of the components of a farming systems research network already exists in the AARD. One can identify specific centers and institutes which carry out one or more of the necessary steps of farming systems research, namely, description, design, testing and extending the results of the research. The present situation could be described as a "passive" approach to farming systems research.

While the components of farming systems research may be present in AARD, a more coordinated effort is needed to institutionalize the farming systems approach within the AARD. To accomplish this would require the improvement or development of better linkages or systems of communication between the various research units engaged in different parts of farming systems research. Figure 2 provides an example of these linkages. Whether or not the research system completely adopts the "farming systems research prospective" has yet to be debated.

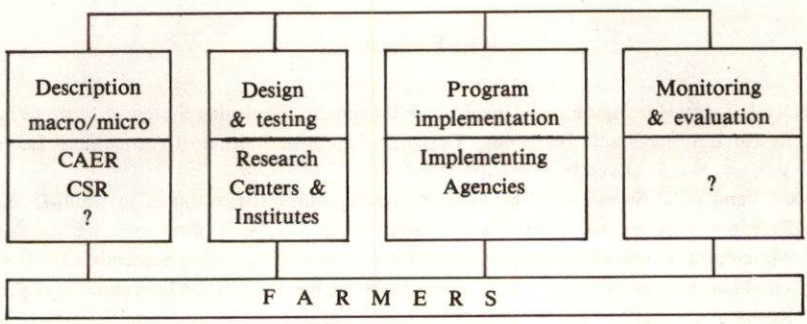

Figure 2. Framework for farming systems network.

MEASUREMENT OF RESULTS OF FARMING SYSTEMS RESEARCH

Farming systems research concentrates on developing new technologies to meet the needs, not only of farm-households, but also for regional agricultural development. Hence, any methods of evaluating the results of farming systems research must include these objectives.

Two impacts are expected to be generated from a farming systems research program, namely, the process impacts and the final impacts. The process impacts will affect institutions as well as economic structures of the study regions. For example, a farming systems program resulting from well defined and integrated research activities will strengthen the linkages among sectors within the framework of regional agricultural development. Furthermore, the program is expected to improve the backward and forward linkages between sectors. The technology being adopted should also generate better use of resources that might be different from one region to another.

In order to measure the final impacts, we must evaluate the effect of farming systems research programs on improving the welfare of society, namely, producers and consumers. This is not an easy task, since we have to be able to identify indicators of achievement. The rate of technological adoption by farmers, production parameters, income, and standard of living of the farmers are among the most important indicators to be considered for evaluating the program. The rate of employment absorption in the region, and indices of commodity dispersion that measures the consolidation process of potential agricultural products within the region, are other possible indicators.

It is clear that no single indicator will accurately measure the final impacts of a farming systems research program. For that reason, quantitative measurements must be supplemented by qualitative insights in order to correct our evaluations regarding the overall impacts of farming systems research programs.

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THE EFFECTIVENESS OF FARMING SYSTEMS RESEARCH AND EXTENSION

by

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1.0 INTRODUCTION

During the past 15 years there has been increased emphasis on applied research on small-scale farms in the developing world for the purpose of satisfying one or more of the following objectives: 1) to increase agricultural output as a national goal, 2) to improve the economic well being of families on small-scale farms, 3) to redress problems of income inequity in rural areas and between urban and rural populations. This concern for small-scale agriculture has surfaced in a period when expenditures on all agricultural research throughout the world have increased at a dramatic rate.

Schultz (1984) has reported (based on work by Boyce and Evenson) that annual world expenditures in constant dollars have increased from $769 million to $ 3,841 million in the period from 1951 to 1974 but nearly doubled again to the estimated level of $7,000 million by the year 1979. This is indicative of the anticipated if not realized high returns to agricultural research and the increasing reliance on agriculture in the development process. One needs to be reminded, however, that some of the arguments for agricultural research in developing countries (including farming systems research) have been based on the returns to research that have been reported for the higher income countries--those areas of the world where the research expenditures have been at the highest levels and where they have been sustained over the longest period of time. Evenson (1984) has shown that there is a direct relationship between public research expenditures as a percentage of the value of agricultural product and the national average per capita income. For example, after classifying countries according to per capita income, he computed that the percentage of agricultural product value spent on publicly supported research was 0.62 and 1.48 percent for the low and high income groups, respectively. Nevertheless, this analysis of a limited number of studies of research productivity in developing countries indicates that the research programs there have yielded returns roughly equal to those for more advanced countries.

Despite the almost religious zeal with which the proponents of agricultural research advance the cause (especially the farming systems research advocates) and the aura of optimism for beneficial results that surround this zeal, there are

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obstacles in the management of national research systems that must be overcome before a spirited optimism will be common place in developing countries.

I would like to call attention to some of the identified problems in national research systems management as an avenue for coming to grips with the more specific issues of implementation of farming systems research projects. There are those who argue that farming research in its intended sense is as much extension as it is research and that to align it unduly with the research establishment is to do it injustice. Many of the pitfalls in research management that have been identified on a world basis are most prevalent in Africa and to a lesser degree in Latin America. The research systems in Asian countries (although not without problems) are put up as the leaders of, if not the models for other countries. Indeed, the Philippines and Indonesia are the envy of many countries. I fail to see how we can intelligently discuss farming systems research as an abstraction from other development institutions--especially those who compete for resources in the area of research.

2.0 ISSUES OF RESEARCH SYSTEM ORGANIZATION AND MANAGEMENT

The most comprehensive treatment of research organizational problems in developing countries with which I am familiar is that of Rohrbach (1984). I side with him when he states that surveying and highlighting the problems will not solve them. But, doing so may provide some of the framework needed for their solution.

2.1 AGRICULTURAL DEVELOPMENT POLICY AND AGRICULTURAL RESEARCH

Investment in agricultural research is one of many thrusts of the development milieu that competes for funds in national development plans. Among other reasons, it is viewed as a means to increase productivity which in turn has the potential for reducing imports, expanding exports, decelerating rural to urban migration, supplying food to the urban poor, reducing unemployment, reducing inequities, improving nutrition and advancing rural living standards. Unfortunately, not all of these goals are compatible nor are all of them the logical consequences of increased productivity. Furthermore, agricultural research, with the potential for increasing output through improved technology, will have a neglible effect if farmers do not adopt the technology. Such adoption requires both farmer understanding of the technology and a conviction on the part of the producer that it will be profitable under his/her circumstances. Government price policy (typically treated as an unchanging parameter in farming systems research) may be far more important that the production function in the farmer's profit equation. Many years ago, a macroeconomist colleague of mine chided me as a "farm management person" for not being more policy conscious. He said "One announcement from the Secretary of Agriculture on a given day that would insure a 10 percent increase in farm product prices would do more to improve farmer

welfare (even with outmoded technology) than all of the ideas for increasing crop and livestock output that you can package in a year". That statement has haunted me all these years--not because I was being depreciated for my professional interests but--because it points to the futility of output-increasing technology research in an unfavorable policy environment.

The influence of national policy on farming systems research projects is usually recognized. We know its importance in such matters as selecting the target area, directing research priorities with regard to type of farm and type of commodity, providing manpower and training, working out the interagency relationships and otherwise providing the political and economic environment for research (Shaner, *et. al.*, 1981). The reverse causality; namely, the influence of farming systems research on national policy, is less understood. This "bottoms up" relationship is, in some way, related to the fact that the individual farm household focus of farming systems research offers little in the way of prescriptions for regional development.

Norman (1983) delineates the Farming Systems Approach to Research (FSR) involving the development and dissemination of relevant improved practices (technologies) and 2) Farming Systems Perspective (FSP) **which influences the development of relevant policies and support systems.** He stresses that "micro-macro" dimensions are possible in both FSR and FSP "but because FSR programs have usually been located in agricultural research institutes--primarily crop oriented, often with poor linkages to planning or policy-making agencies--the FSP has usually not been operative". (Norman, 1983, p. 3).

Hildebrand and Waugh (1983) have followed Norman rather closely in delineating the micro and macro aspects of farming systems research. The more macro aspects of technology research and development are encompassed in the terminology of "the farming systems approach to infrastructural support and policy" (FSIP). The more limited scope, with emphasis on research within the farming system, is referred to as "the farming systems approach to technology generation, evaluation and delivery" (FSR/E). They propose that FSIP is applied, farmer-oriented **socio-economic** research supported by the agro-biological sciences in a team effort and that FSR/E is applied, farmer-oriented **agro-biological** research supported by the socio-economic sciences in a team effort which includes extension responsibilities. These are interesting concepts but offer little content on the nature of the two-way policy feedback system in farming systems research projects.

The CIMMYT Economics Group (1984) has emphasized the importance of information feedback from farmer to research institutes going so far as to say that "the information fed back to the experiment station is often as important as the technologies recommended to farmers" (p. 368). They also point out that the information gleaned from the survey activities of the project provide a special form

of intelligence system for policy-makers by detecting bottlenecks such as untimely delivery of inputs, acute insect populations, lack of improved seeds, etc.

Timely delivery of information to policy-makers is one of the potential benefits of farming systems research. Martinez (1983) indicates that the addition to the national stock of knowledge through increased capabilities and gains in experience of the scientists in the on-farm research undertaking are valuable outputs from the research process. One could add the derived gains in knowledge and experience on the part of farmers as well. But, because of the intangible nature of these externalities and because of the lack of a market or other mechanism to measure how much society values these kinds of products, they are essentially by-passed in the evaluation methodology. Instead, the incremental value of "before and after the adoption" of the new technology is used as a proxi of the social gains of applying on-farm research technologies. Failure to make a more serious attempt to assign value to the externalities in agricultural research, or alternatively the failure to ascertain an estimate of the subjective priority rating given by policy-makers to these externalities, may be offered as one hypothesis as to why the policy dimension of farming systems research has taken a back seat. Another hypotheis could be that analysts who are capable of making the bridge between macro and micro policy are extremely hard to find.

2.2 FUNDING FOR AGRICULTURAL RESEARCH

As already mentioned, funding for agricultural research worldwide has increased rapidly in recent years but commitments to research on the part of governments in developing countries has been relatively low (in terms of their agricultural Gross Domestic Product). Rohrbach (1984) citing Anderson (1981) discusses five hypotheses to explain low government commitments. The first is that politicians and planners have not been convinced that past research achievements and on-going investigations have contributed substantially to national development. This attitude prevails despite the reported high returns to research investment elsewhere. It can be likened to the inertia of farmers who are slow to commit resources to a risky investment even with the evidence that others have experienced very favorable returns.

The second hypothesis is that governments looking for immediate solutions to their most pressing needs will avoid the funding of investments of a long term nature such as is required to build strong agricultural research institutions. Wherever political instability exists, small returns on short term investments will be favored over large returns from long term investments. Rohrbach offers the suggestion that this hypotheses could explain why, in many developing countries, the investment in extension services greatly exceeds that for research.

The third hypothesis offered by Anderson is that politicians favor physical visible "monuments" to development. Even though small incremental improvements for a large number of farmers would compute to a significant national

achievement, these benefits may be difficult to employ as a basis for rallying political support. In interpreting this phenomenon, Rohrbach speculates that this could explain the proliferation of research institutes as physical entities even when there are insufficient operating resources to support them.

The fourth hypothesis is related to the third, and to the adage that "the squeaking wheel gets the grease". The political voice of small scale farmers is weak and whether their organizations have been viable or not, they have not rallied for agricultural research. If there has been a voice that would affect research resource allocation, it has come from the large commercial farmers who are supportive of the government policies. One of the justifications for the farming systems approach to research is that it provides a place for small scale farmers in the national research establishment. They are being offered this privilege whether they asked for it or not.

Lastly, there is the hypothesis that agriculture simply does not stand high enough in the ordering of national priorities to warrant long term research support. It is true that a low food price policy can dampen the most optimistic expectations from agricultural research. But, I find this hypothesis the least persuasive of the five in the light of the place occupied by agriculture in most national development plans.

In the final analysis, the way research expenditures are used rings more importantly than the absolute level.

According to Oram and Bindlish (1981) and presented by Rohrbach, about 15 percent of annual research expeditures are devoted to capital costs with most of remaining being used to cover staff salaries--leaving little to meet operational expenses. While working in the Philippines on the Technology Package Project (funded by USAID and the Philippine Government) I was talking with the President of Central Luzon State University (implementing institution) about the impact of the project on the administration of his university. My recollections of that discussion is that the project raised havoc. According to President Compos, the foreign donor contributions to the university (representing foreign exchange capital investment and equipment among other things) were incorportated into the university budget. By decree, universities at the time were expected to prepare zero based budgets without net increases. As a consequence of including the project costs in the university budget, President Compos said (perhaps in jest, but with a straight face), "I don't have enough operating money to buy any additional supplies or small equipment for this university--not even a small hand pushed lawn mower".

Speaking of research in Central America, Pineiro and Trigo were quoted by Omar and Bindlish (p. 52) as saying, "Professional staff, in general, are not being used effectively and one important reason is the lack of budgeting support for operational costs. There is a limit to the number of investigations that can be

properly supported, and new ones should not be added unless there is a firm commitment to provide the budget to support them".

The current wave of farming systems research in developing countries is funded to a large extent by foreign assistance. The recurrent cost deficit problem goes largely unnoticed as long as the projects are under contract. But, what happens when the projects terminate? A couple of years ago I was on a team to provide assitance to the Pakistan Government and the USAID Mission in the preparation of a Project Identification Document for a farming systems research and extension project. We visited regional research stations and extension offices throughout the country. At one station we were advised by the researchers there that they already had experience with on-farm research through an earlier project funded with foreign assistance. Although the project had terminated, they were struggling to carry on. The original vehicles for the project were in need of repair -- some were inoperative -- and their research on farms, of necessity, was restricted to those farms closest to the station even though they were atypical. Their plea was for the donor community to play a larger role in helping meet operating costs than has been the case in the past.

Norman (1983) makes two suggestions for institutionalizing farming systems research under conditions of tight resources: First, avoid any attempts to perpetuate a self-contained project. Rather, early in the implementation process, efforts should be made to integrate and cooperate with other groups/organizations/projects within the national program -- even at the expense of the short run gains that would accrue to working independently. This would need to be a conscious effort, because, to satisfy the local government and donor alike, there is pressure to show positive results in as short a time as possible.

The second suggestion is simply to apply the criterion for resource use in farming system projects as follows: "Don't expend resources to learn all that is desirable to know, but rather use them at the minimum level of resources needed in order to obtain useful results".

2.3 MANPOWER DEVELOPMENT FOR AGRICULTURAL RESEARCH

Lack of adequately trained scientists is one of the most critical restraints to research system improvement thoughout the developing world. However, the problem of numbers is less acute in Asia where it is claimed (Oram and Bindlish, 1981) that 50 percent of all of the scientists in developing countries are to be found. But, as Rohrbach (1984) has pointed out, the manpower issues include far more than numbers and their disciplinary representation. "Low salaries and professional incentive have brought high manpower, turnover thus threatening what limited research capabilities have been established". The orientation of staff training has not always been congruent with the problem solving priorities of most smaller programs. The importance of strong managerial and technical support has gone largely unrecognized in relation to concerns regarding program scientists.

Relatively few programs have well defined long term manpower development plans, and many short term plans have been poorly coordinated with overall investment strategies guiding the allocation of research resources. As a result, staff scientists have been employed without adequate funding to carry out productive research" (Rohrbach, 1984, p. 85).

The main point that I would like to make here is that manpower issues take agricultural research in general and farming system research in particular out of the realm of a "quick fix" for development and to the reality that growth from research is a long-term enterprise. Eicher (1984), in his insightful analysis of the food crisis in Africa stated, "Agricultural development is a slow and evolutionary process, and it is up to African states and donor agencies to jettison the crash project approach and start now to lay the foundation for long-term investments to solve the food production and poverty problems over a 10-20 year period". (Eicher in Eicher and Staatz, 1984, p. 475). While there is a wide gap between the level of development between Asian and African countries, I subscribe to Eicher's implication that crash programs can interfere with an orderly process of development -- even in Asia.

Having inferred that I may be regarding farming systems research as another crash program I feel I should digress for a moment to discuss the quick payoff intent of most farming systems research and its justification.

There is a sense of urgency in most farming systems research projects to show tangible results in terms of increased productivity, improved farmer living conditions and returns on the research investment. One of the reasons for such urgency is the political riskiness of a new venture that can best be overcome by quick positive results. The way to counter the reluctance to divert research resources to a new undertaking and the skepticism of administrators and country scientists is to achieve success in a hurry. There is a tested interest on the part of participating national institutions, donor agencies and the scientists involved to demonstrate, in as short a time as possible, the merits of the approach, lest the skeptics be right and future funding be curtailed.

The sense of urgency is also related to the charge that "traditional research" has been unsuccessful, even over a long period of time, to generate the type of technology that is both needed and desired by farmers. The farming systems approach to research is offered as a response to this problem. Short-cuts in technology development and diffusion are possible with improved problem identification with farmer participation followed by meaningful research design and testing, and with improved linkages with the national/regional extension services and with other research institutions. However, research that would entail too much complexity in design or the consideration of alternatives that would take too many years to evaluate should be avoided for fear that it be subject to the same charges of ineffectiveness that have been leveled by "traditional research" in the past.

The need for quick results carries with it the need for simplicity (as indicated by the number of variables to be studied) in the way problems are prioritized and in the way the research is designed, carried out and evaluated. The CIMMYT Economics Group (1981, p. 344) has addressed the apparent anomaly between using a holistic view of an often complex farming system and using a research methodology that emphasizes quick results and cost effectiveness. The argument includes the following points: (1) To satisfy farmer needs, the technology should avoid the requirement of a significant increase in farmer capital outlay, should entail a minimum of risk and should favor a cautious learning process, (2) Small farmers are not receptive to farm reorganization but rather favor simple marginal adjustments in their current system, (3) A narrow focus better accomodates national priority themes than does a broad-based, complex research focus, and (4) Long run improvements in the agricultural production system are best accomplished through a sequence of marginal adjustments.

I find these points to be very persuasive arguments for simplicity and especially suitable for research institutions whose research mandate is confined to the improvement of the performance of a limited number of farm enterprises in their relation to other components in the farming system. The work by CIMMYT and IRRI has contributed much to the refinement of the methods of farming systems research. They are especially appreciated for the integration of their programs into the on-going efforts of the national research establishments. Hence, farming systems research need not be classed as "quick fix" projects but there is need for realistic expectations as to what is possible with a project life of three to five years.

Let us now return briefly to the research manpower issue -- a long term problem in development. There are many questions in this area with bearing on the implementation of farming systems research projects for which we have no answers. What are the losses associated with the undertaking of a farming systems research project which must, for lack of trained country counterparts, be conducted in its initial stage primarily with expatriate personnel? What is the nature of the losses associated with the high mobility of the most qualified scientists who either move laterally across several successive projects or move upward in the institutional hierarchy or are attracted to the riches of the private sector? What is the optimum level of training to sustain a viable research program over time? To this question one can note that, in the earlier years of the farming systems research undertaking, heavy emphasis was placed on training technicians at the baccalaureate degree level in large numbers with a subdued interest in training at the graduate level. Now, the pendulum is swinging in the opposite direction to fill the needs for Ph.D. degree holders in a growing research establishment. Oran and Bindlish (1981, p. 2-5) called attention to the World Bank goal of 20 percent of the technical manpower requirements being comprised of

Ph.D's in developing countries when the present proportion of Ph.Ds' is roughly at the 9 percent level.

What are the consequences for research accomplishment if low salaried researchers are encouraged to "moonlight" resulting in serious research being a part time activity? What kinds of incentives are necessary to encourage researchers to participate in the arduous and time-consuming activities of on-farm research in preference to doing on-station research or administration?

3.0 THE IMPLICATIONS OF INTERDISCIPLINARY RESEARCH

The manpower development problem has been exacerbated by the need to staff farming systems research projects with technically competent staff from several disciplines. What is the optimum number and mix of disciplines for an effective farming systems research project? How do the requirements for administration of interdisciplinary research differ from those of disciplinary research? How do incentives and rewards for performance in interdisciplinary research differ from those of disciplinary research? What are means for developing skills needed for team research when the training for scientists has been for a single discipline? Has the interdisciplinary aspect of farming systems been overemphasized? Alternatively, has the interdisciplinary aspect been more lip-service than practice in farming systems research? What bearing does the unit of analysis have on the interdisciplinary mix?.

The most obvious deduction that we can make about the composition of an interdisciplinary research team is "the narrower the scope of the research agenda and the greater the simplicity, the fewer is the number of disciplines that are required". This is not to say that research based on simple experiments after looking at the system as a whole cannot make effective use of specialists from various disciplines in the several methodological processes from site selection to the diffusion of tested results. Nevertheless, if for example, the technology focus is on the agronomic aspects of the farming system, a research team composed of an agronomist and a micro-economist may serve the project goals very adequately as has been demonstrated by CIMMYT (Collinson, 1982).

At the other extreme, we can turn to agricultural universities that are involved in technology development to find not only a broad based mandate for technology development but a wide spectrum of disciplines involved in the process.

I was involved for two years with the Technology Packaging Project at Central Luzon State University (CLSU) in the Philippines. Rather than searching for a homogeneous farm population (recommendation domain), the project design was directed toward the diverse technology needs of the entire farm population located within a 15 kilometer radius of the university. This target population represented a very heterogeneous mixture of farm types, production and marketing conditions, and technology development needs in both irrigated and rainfed areas. The university response to this situation was to muster the university faculty for an

attack on a broad front. The core disciplines involved in the attack included animal science, agricultural engineering, agronomy, agricultural economics, rural sociology and development communications. Other disciplines of the university were utilized as needed. The approach called for concerted interaction with groups of farmers representing the several substrata in an attempt to prioritize research needs in response to a range of expressed needs. These needs were combined with available "shelf technology", stated national priorities and with the creative competencies of the interdisciplinary team engaged in the undertaking. The strategy was to make available a wide range of potentially viable alternatives from which local farmers could select, evaluate and test under the supervision/guidance of university personnel. This was done in preference to isolating a primary commodity or farming system because of the university's commitment to serving a diverse clientele and because of its political sensitivity to the different needs of people under different social and economic circumstances. In this setting, far less emphasis was given to generating quickly utilizable technologies than was given to demonstrating to the community at large the long term development potential of having the rural people and the university working in concert and in a continuous learning mode to address the everchanging needs of the area.

But, universities present unique situations for observing the problems of interdisciplinary research. The pressures that result from overloaded teaching schedules and normal university duties combined with a typical reward system which calls for publication in ones own discipline make necessary special administrative leadership. The leadership in the CLSU case was strong and promoted interdisciplinary productivity by providing financial incentives, released time and opportunitites for special training and travel on condition that the team functioned according to agreed upon objectives and procedures. Failure to perform could mean severence of special privileges and a "return to the ranks".

The approach taken at Chiang Mai University was quite different for achieving interdisciplinary harmony. The initiative was taken by a core of CMU faculty who had years of sometimes frustrating experience working on the Multiple Cropping Project. The growing evidence that the attempted extension of university developed technology directly to the village was not achieving the desired results was enough to make some of the researchers press for an improved systems perspective with a stronger orientation to the family needs and to the potential of the region. However, they found that the solution was not to be found in simply having representatives of the various disciplines expound the merits of their domains respectively. Rather, they chose, under the tutelage of an expatriate ecosystems specialist, to permit system science, itself to be the unifying language for interdisciplinary research (See Conway, undated and Chiang Mai University 1980). It is the only project with which I am familiar where researchers working on technology development have gone beyond casual references to systems terminology and made the effort to learn what systems analysis was all about.

So we conclude that there are no magic solutions to the problems of conducting applied research in a team approach. They are so intertwined with institutional short term and long term objectives, the amount and quality of available resources and the complexities of human nature that each case is a special one.

Perhaps we have even placed too much emphasis on the interdisciplinary aspect of farming systems research. I am very supportive of Schultz (1984) in his position that "It is all too easy to become enamored with the phrase interdisciplinary research. What matters is the actual value that is to be realized from the interaction among scientists who have different professional skills. So-called interdisciplinary research is as a rule weak on theory and soft in the quality of research that gets done. The effects of various incentives on research workers' productivity is of major importance. The built-in incentives that characterize organized agricultural research in most low-income countries are bad. All too often agricultural scientists are worse off in this respect than high class clerks in the bureacracy of the government" (p. 343).

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